

The Representative Agent in Microeconomics: A Samuelson-Koopmans Thread?

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Abstract

This article asks whether the representative agent might have emerged as the outcome of transformations that occurred in microeconomics in the 1930s and 1940s, specially, in the subfield of demand theory. To tell this story, I begin with a particular historical interpretation of this subfield, propounded by Wade Hands and Philip Mirowski in the 1990s, known as the Hotelling-Schultz Impasse. Although this impasse was abandoned by the end of the 1930s, the testing of the Symmetry Restrictions and the validity of the Integrability Conditions continued to draw the attention of different research centers. The Cowles Commission, and its research director, Tjalling Koopmans, played an important role during this stage and, more to the point, in the subsequent emergence of the representative agent in microeconomics through their approach to aggregation problems. The significance of Paul Samuelson's introduction of homothetic preferences into General Equilibrium theory and its connection to Koopmans's writings during the 1950s is also emphasized.

Keywords: Representative Agent, Aggregation Problems, Tjalling Koopmans

Resumo

Esse trabalho investiga se o agente representativo pode ter surgido como resultado das transformações que ocorreram na microeconomia nos anos de 1930 e 1940. Para construir esta narrativa, eu começo com uma interpretação específica proposta por Wade Hands e Philip Mirowski nos anos de 1990, conhecido como o Impasse de Hotelling-Schultz. Embora esse impasse tenha sido abandonado na década de 1930, o teste de restrição de Slutsky e a validade da condição de Integrabilidade continuaram a chamar a atenção de diferentes centros de pesquisas. A Comissão Cowles e seu diretor de pesquisa, Tjalling Koopmans, tiveram papel importante durante este estágio e, mais importante, no aparecimento subsequente do agente representativo na microeconomia. A significância da introdução das preferências homotéticas por Paul Samuelson no arcabouço de equilíbrio geral e sua conexão com os trabalhos de Koopmans na década de 1950 também é enfatizado.

Palavras-chave: Agente Representativo, Problemas de Agregação, Tjalling Koopmans

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Área 1 - História do Pensamento Econômico e Metodologia

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1 Introduction

Samuelson is omnipresent in American and even world economics; (...) In the pages of every journal and many a collective volume he appears, read equally by esoteric specialists in optimal growth or integrability conditions and by the most policy-oriented in practical central banking, impatient with theoretical niceties.

- Kenneth Arrow, 1967

It has become popular among economists that a major result in microeconomic theory is consumer rationality. The path until this outcome begins with axioms of preference, proceeds with existence proof of a utility function and continues with the solving of a (constrained) *utility maximization* problem that leads to Marshallian demand functions. Such functions are said to satisfy some desirable properties. It is possible to attain an equivalent result in a (constrained) *expenditure minimization* problem where, this time, Hicksian demand functions are obtained.

The connection between Hicksian and Marshallian demand functions owes to an expression proposed by Eugene Slutsky, a fundamental equation that breaks quantity demanded of a good - due to variation in its price (or in another good's price) - into substitution and income effects. Symmetry of the Slutsky matrix, in conjunction with Integrability Theorem, guarantee that Marshallian demand functions reverse to the axioms of preferences, thus, connecting the outcome of utility maximization problem to its initial definitions.

Yet, the development of microeconomic theory faced more resistances than the history presented above (and elsewhere) suggests. A more careful look into the works as well as careers of the economists involved might allow one to better observe how, operating in networks, they created, extended or even ignored theoretical and/or empirical research whose outcome are not reflected in the canonical history of the subject. The aim of this article is to tell one of these stories.

My purpose in this essay is, therefore, to investigate if and how the representative agent might have been the outcome of changes in microeconomics that occurred prior and during the Second World War. Moreover, as Kenneth Arrow reflected in his review of Paul Samuelson's *Collected Scientific Papers*, back in 1967, my aim is also to show that a significant part of Tjalling Koopmans's research in microeconomics was informed by Paul Samuelson's research in the field, and this was especially so concerning the representative agent. To substantiate this claim, I begin this narrative with an important, yet overlooked episode in the development of microeconomics in the 1930s, the "Hotelling-Schultz dialogue," a research effort that consisted in, *inter alia*, testing the symmetry conditions of Hotelling demand functions. With the advent of contributions from Eugene Slutsky, the Cowles Commission did more than testing the new symmetry conditions: under the influence of the Keynesian revolution, the well known Slutsky income effect - defined at the microeconomic level - was used to shed light on facts defined at the macroeconomic one, an objective of the Slutsky-Walras program that engaged Koopmans since the 1940s.

Apart from this introduction, this article is organized as follows. In the first two sections, 2 and 3, I present a brief overview of the history that gave rise to the investigation in this chapter: first, I introduce a certain stylized version of demand theory, as generally seen in microeconomics textbooks, then, followed by a more intricate account on the subject provided by Wade Hands and Philip Mirowski. In section 4, I take on the claim in the last paragraph, explaining why the Slutsky-Walras program was centered on the problem of aggregation and, above all, how from this effort a few considerations about the representative agent can be inferred. Section 5 connects Koopmans's research efforts as discussed in the previous section to Samuelson's contributions in microeconomics which also helped give rise to the representative agent. To complement the aim of this article, in section 6, I consider the meaning of dynamics in economics, a topic originating concomitantly in a important paper analyzed in this essay (Samuelson, 1943), with interesting implications to the interpretation of the representative agent. In section 7, I offer some concluding remarks.

2 The History of Classic Demand Theory Revisited

It is rather common to observe microeconomics texts flirting with history before getting to the contents proper, even if briefly. This is the case of the graduate textbook, “Advance Microeconomic Theory,” by Geoffrey Jehle and Philip Reny, that stated, “Before we begin, (...), a brief word on the economists’ thinking will help to place what follows in its proper context” (Jehle and Reny 2011: 4), with the authors going on to present the evolution of preferences and utility concepts in historical perspective from Vilfredo Pareto all the way to Gérard Debreu, including still the contributions by Eugene Slutsky and John Hicks.

Following this reasoning, in the manual, “Economics and Consumer Behaviour,” Angus Deaton and John Muellbauer (1980: 61) wrote: “Although we make no attempt to provide a historical survey of applications of demand theory, the studies will be presented in rough chronological order.” More examples of this approach can be found in the literature, perhaps more predominantly in the field of macroeconomics.¹ The aim of this section is to raise objections to such interpretations, in an attempt to answer: “How well do we know about the history of demand theory?”

In the spring of 2005, a three-day History of Political Economy Conference was held in North Carolina, United States, to discuss a central concern in microeconomics. In the words of the organizers, “The conference provided participants with an intense and spirited exchange on a wide range of topics related to the history of demand theory and its related topics” (Mirowski and Hand 2006: 3).

The scholars engaged in the meeting wanted to produce a written volume capable of signposting the community beyond the uninvolved “two- or three-acts stories,” typically associated with the topic. Notwithstanding this new research effort have begun to render today’s demand theory a direct upshot of a certain *stabilization process* of mid-twentieth century, the standard narrative continued to claim an old tale. According to it, after demand theory became part of the neoclassical enterprise in earnest around 1875, it took the theoretical framework only few sequential turns (or “play-acts”) to reach the present stage.²

In such a smooth and stylized portrayal, the first act took place circa 1850-1875: it was about its origin in hedonistic psychology, i.e., “(...) rested on subjective introspection rather than objective observation” (ibid, p. 3), a stage where cardinal measurement of utility played a mayor role and a phase associated with such names as Jonh Stuart Mill and Jeremy Bentham. The second act was the ordinalist revolution of the 1930s, marked by the elimination of hedonistic features and made possible with foundational contributions from Vilfredo Pareto, John Hicks, Roy Allen, Eugene Slutsky, and others.³ Finally, if it was a breakthrough of its own, then, Paul Samuelson’s revealed preference theory would consist in the third turn, a method which seemed to give a stronger scientific basis to the classical theory of demand (installed after the second turn), for it turned consumer’s choices into an observable behaviour and no longer dependent on abstract and unobservable axioms of preferences.⁴

Investigating the version by which stabilization in demand theory happened in the 1930s-1950s decades leads one to a way more convoluted task, yet one whose resulting account could possibly

¹See Acemoglu (2009), Blanchard and Fischer (1989), Romer (2001) and Barro and Sala-i-Martin (2004).

²“Marginalism,” a late nineteenth-century movement associated to a new form of analyzing demand, is sometimes used interchangeably with “neoclassicism” or “neoclassical school.” See Ekelund and Hébert (2002) for an analysis of neoclassical microeconomics.

³Lenfant (2006) defends an alternative view in which the notion of ordinalism was not a guiding principle in the prestabilization era. For another informative article on the ordinalist revolution, see Hands (2011).

⁴Samuelson’s contribution was mostly due to his 1938 article, “A Note on the Pure Theory of Consumer Behaviour,” published in *Economica*. There is a debate in the literature whether Samuelson’s Revealed Preference approach was as significant as the ordinalist revolution or simply “(...) a further refinement in the ordinal utility program (a way of empirically ‘revealing’ the consumer’s preference” (Mirowski and Hands 2006: 3). Stanley Wong’s (2006) thesis is that Paul Samuelson knew since the very beginning that his weak axiom of revealed preference would not be enough to derive the law of demand and, thereby, his research program would not be an alternative to the preference-based approach to utility theory.

supersede the last two turns above. Further, if accurately told, it would cast light on the intellectual and methodological origins of three related teaching and research institutions in the United States, lending us a view into how - at least as far as the theory of consumer choice is concerned - the current orthodox approach came to be. This is why I decided to follow Hands and Mirowski (1998) and Mirowski and Hands (1998) for whom, “To reduce the motley of prestabilization American economics to manageable proportions, we will focus on a particular subset of economists who shared some similar theoretical orientation” (Mirowski and Hands 1998: 261).

All such similar theoretical orientation aimed to tackle, to a greater or lesser extent, a problem originated in the so-called Hotelling-Schultz dialogue. By doing so in their unique ways, Samuelson’s revealed preference theory and the approaches advanced both by the Cowles Commission and the Chicago School produced a theoretical core during the 1938-1955 period that makes up most of today’s orthodox neoclassical demand analysis, “a single monolithic homogeneous neoclassical strain” (ibid), though never forming a single theoretical body.

What will come out of this studies are the contributions from the two exponents in this essay. The first one is Paul Samuelson, a personage who not only played a supporting role in the Hotelling-Schultz impasse, but almost single-handedly provided a solution to it. The second personage is Cowles Foundation’s Tjalling Koopmans, a key contributor in the Foundation’s normative turn of rational choice theories in the 1943-1954 period (Herfeld 2018).

3 The Empirically-motivated Hotelling-Schultz Dialogue

3.1 The Context

Although the number of empirical research in economics began to display an increasing trend as early as the 1970s, it took hold in the profession only in the 1980s for several reasons. Among these were the ever-increasing availability of data, the development of new statistical and econometric methods and the reduced cost of computer power, to which one might add a higher demand for works of “practical relevance.”⁵

Even profiting from a whole new set of scientific and technological advances certain field has yet to see the expected improvement: in demand analysis, economists continue to face hurdles of theoretical and empirical nature and, as the story involving Harold Hotelling and Henry Schultz will show, such a state goes back at least to the years before World War II. In hindsight, as I shall attempt to argue, it exemplifies what Andrew Pickering defined *dialectic of resistance and accommodation*, a (scientific) process in which the representative agent is precisely what has been taken out of the mangle machine.

In their quest to scrutinize the rise of orthodox demand theory in the United States in the first half of the twentieth-century, Wade Hands and Phillip Mirowski have identified a point of origin, a sort of theoretical *tabula rasa*, where two “relatively minor figures” initiated a scientific inquiry that ultimately determined the direction of few, yet influential economic research centers in the United States.⁶ In the authors’ interpretation, everything began when Henry Schultz and Harold Hotelling set out on a task in the 1930s to find, *inter alia*, a theory capable of underpinning the empirical

⁵Few papers have recently analyzed the *empirical turn* that occurred in economics over the last forty years. Combing through nearly 33 journal titles cited in selected years of the *American Economic Review* and using *machine learning* algorithm, Angrist et al. (2017) concluded, among other things, that the profession’s turn toward empirical research has been an evolution within instead of across fields. The volume edited by Backhouse and Cherrier (2017) provides various views on the rise of applied research in historical perspective.

⁶The two papers written by Wade Hands and Phillip Mirowski were “Harold Hotelling and the Neoclassical Dream” and “A Paradox of Budgets: the Postwar Stabilization of American Neoclassical Demand Theory,” both published in 1998. Though the authors changed the order of their co-authorship, I will simply refer to “Hands and Mirowski” (in line with the satisfaction of the alphabetical protocol) when referring to the “Impasse” and the correct order when specific passages of an original article are used as references or, obviously, quoted.

demand functions they had been - painstakingly - trying to estimate.

The first personage in this story, Henry Schultz, was born in 1893 in a Russian-occupied Poland.⁷ After attending college at the City University of New York, Schultz began graduate work in economics at Columbia University under the early econometrician Henry Ludwell Moore, writing a dissertation on the statistical estimation of demand functions.⁸ With the conclusion of the doctoral degree, Schultz was appointed professor of economics at the University of Chicago where he carried out the major work of his career by founding a statistical laboratory to deepen the estimation of demand functions, “(...) a large program of research of a kind definitely projected by earlier studies” (Hotelling 1939: 98).⁹

In need of finding a reasonable theory behind the estimated equations, Schultz began to look in the general equilibrium approach pioneered by Leon Walras and Wilfred Pareto. In his mind, such functions not only had pervasive interdependence at the market level (thus the *general equilibrium* character), but more to the point, were the result of agents’ utility maximizing behaviour (exactly *à la Walras*). As emulating the natural sciences, there had to be “laws” behind empirical regularities in microeconomics as well. What Schultz did not realize at the beginning, nonetheless, was how difficult his applied task would turn out to be, let alone contribute to the theory he so dearly praised.

A second contributor in those early studies of empirical demand analysis and utility theory was Harold Hotelling, raised in Seattle, Washington, but born in Minnesota in 1895. Although graduating in journalism in 1919, he went on to find a more fulfilling career in mathematics, earning a master’s degree in 1921. Three years later, Hotelling completed a doctorate also in mathematics at Princeton University with a dissertation on *analysis situs*, known nowadays as topology, a field where his adviser - Oswald Veblen - contributed with the first ever modern treatment of the topic (Darnell 1988; Mac Lane 1964).¹⁰ As he would later recall of this graduate experience, after being rejected by the graduate program in economics at Columbia University, a fact that postponed his immediate desire to apply the science of mathematics to uncover “new truth” in economics, studying subjects as wide as mathematical physics, differential geometry and astrophysics gave him an even better grasp of the tools, which turned out to be helpful in his subsequent works (Darnell 1988).

Leaving university in 1924 with jobs for pure mathematicians in short supply, Hotelling debuted his professional life at the recently created Stanford Food Research Institute (SRI) as a mathematical and statistical consultant (Hands and Mirowski 1998). It was at the institute, collaborating with his colleague Holbrook Working, and mirroring Schultz’s experience with widespread economic crisis and uncertainty (especially in the food sector), that he developed his own interests in demand theory and estimation methods.¹¹ In fact, as Arrow and Lehmann (2012: 903) put it, “It was during his

⁷His family emigrated to New York City, United States, when he was still a child. See Hotelling (1939) for more information on the life of Henry Schultz.

⁸In a Festschrift to Schultz published after his death, Harold Hotelling (1939: 98) wrote: “[Schultz’s] inspiration was in the work of Henry L. Moore, whose pioneer attempt to derive demand curves from time series stirred his enthusiasm.” Henry Moore (1869-1958), considered one of the founding fathers of econometrics, earned a PhD in Economics from John Hopkins University. Spearheading a generation of Americans who crossed the Atlantic to learn from fellow Europeans, Moore took classes in mathematical statistics from Karl Pearson at the University of London and also corresponded with luminaires of that time such as Alfred Marshall, Ysidro Edgeworth, Vilfred Pareto and Léon Walras. On this and additional details of Moore’s career, see Mirowski (1990).

⁹Schultz taught at the University of Chicago from fall of 1926 until his untimely death in 1938. His name is often associated to the early formation of the Chicago School of Economics.

¹⁰Oswald Veblen (1880-1960), nephew of the Institutionalist Thorstein Veblen, earned a PhD in mathematics at the University of Chicago in 1903. A famous geometer of his time, Oswald taught at Princeton University during his whole career. Also at Princeton he helped found the *Institute of Advanced Study’s* School of Mathematics, a place famous for its faculty who included such names as Albert Einstein, John von Neuman and Hermann Weyl

¹¹Holbrook Working earned a PhD in Agricultural Economics from the University of Wisconsin-Madison in 1921 and joined SRI in 1925. Having made substantial contributions in the statistical analysis of commodity prices (JBES 1986), his influence on Hotelling’s 1932 article can be gauged in the several times he was mentioned. Holbrook and his younger sibling, Elmer Working, were also one of the first economists who underscored the importance of *endogeneity problems* in the estimation of demand functions. Elmer’s article, “What do Statistical ‘Demand Curves’ Show,” published in

Stanford period that he began to focus on the two fields - economics and statistics - in which he would do his life's work."

As was the case of some academicians in the United States who used to travel abroad to learn from their counterparts, Hotelling spent six months in England in 1929 working with a leading statistician, making him, "(...) one of the few Americans who in the 1920s realized the revolution that R. A. Fisher had brought about in statistics (...)" (ibid.). So long-lasting was this influence that even following the relocation to Columbia University in 1931, "(...) most of his energy during the 15 years there was spent developing the first program in the modern (Fisherian) theory of statistics" (ibid.), a fact that certainly helped him hone his publications in microeconomics. The bulk of his involvement with Schultz, therefore, took place during this Columbia period precisely when, as editor of *Journal of Political Economy* in 1932, Schultz received an article written by Harold Hotelling.¹²

As I have mentioned above, Henry Schultz sought answers to two specific questions related to the measurement of demand. First, could the Walrasian model fulfill the theoretical void he saw in the estimated curves? Second, since the reexamination of the theory's mathematical foundation was perceived as essential to comprehend the causes of the great depression, in which direction should it be carried out? Schultz found a perfect interlocutor in Hotelling, for his main article, "Edgeworth's Taxation Paradox and the Nature of Demand and Supply Functions," published in the influential *Journal of Political Economy* in 1932, tackled the dual objective above.¹³

Even though Hotelling searched for a more theoretical approach, while Schultz was "(...) the champion of an empirical operationalism" (Hands and Mirowski 1998: 363), they shared the following features:

1. Concerned with practical matters, i.e., the reality of the American countryside in the years leading to the great depression of 1929: thus, the estimation of demand functions were considered paramount to better comprehend hidden events crippling the sector.¹⁴
2. Believed that economic theory was important only insofar as it shed light on tariffs, subsidies, production quotas, and related matters.
3. Particularly on the issue of pure theory, the neoclassical price version had to be:
 - 3.1 Grounded on mathematics.
 - 3.2 Empirical: data should corroborate the theory.¹⁵
 - 3.3 Somehow derived from Physics: "a true scientific method."
 - 3.4 The basis of everything else in economics.
 - 3.5 Conducive to the obtainment of interdependent demand function.

A central takeaway of this section is that, ontologically, both authors believed in the fact that demand functions "existed out there in the world" in an interdependent form, "(...) functionally linked together by rational adjustments to other prices, incomes, as well as various accidents" (Hands and Mirowski 1998: 245), unlike the Marshallian approach in which a simple price-quantity plot would be acceptable. An important part of the following development in this literature then revolved

1927, remains even today a reading source in applied microeconomics courses such as Industrial Organization. Deaton and Muellbauer's *Almost Ideal Demand System* is built, among other models, on Holbrook Working's *Statistical Law of Family Expenditure*, published in 1943.

¹²As Hands and Mirowski (1998) have reminded us, Hotelling and Schultz had already exchanged correspondence as early as the 1920s, but on other matters.

¹³The University of Chicago's economics department, where Henry Schultz was a Professor, has been publishing the *Journal of Political Economy* since 1893. See the volume edited by John List and Harald Uhlig (2017) on the combined influence on our profession of the "Chicago economics" and its flagship journal.

¹⁴As the Stanford University Historian David Kennedy once wrote: "Herbert Hoover needed no comprehensive study to know that the farm issue was urgent. Virtually his first act as president, even before he commissioned his wide-ranging examination of recent social trend, was to convene a special congressional session to resolve the farm crisis" (1999: 17-18). Then, he continued, "But as the agricultural depression of the 1920s merged with the great depression of the 1930s, (...) the misery of rural America knew no relief" (ibid: 19).

¹⁵Both authors pioneered the applicability of linear regression analysis as well as Fisherian statistical hypothesis testing to demand theory.

around a key test called *symmetry condition*. But as much as it was economists' job to correctly find interdependency empirically, and better, base them upon the writings of Leon Walras, Vilfredo Pareto and Francis Edgeworth, things did not work out well. Neither in the past, nor in the present.

3.2 Everything Came Down to Symmetry Conditions: past and present

In the section 5 of the “Edgeworth’s Taxation” paper, Hotelling demonstrated that a certain *symmetry condition* should hold when one took the cross-partial derivative of demand functions with respect to prices, a feature he alternatively dubbed *integrability conditions*. After reading the article:

(...) Schultz immediately set his laboratory to work calculating the demand functions and estimating the partial derivatives to test for the symmetry condition of the Hotelling Economy, as well as the quasi-correlation coefficient to measure complementarity (Hands and Mirowski 1998: 355).

Such an attempt to test for the restriction was probably one of the first carried out in microeconomics, and following its transformation “to the status of core empirical content,” the authors continued (ibid):

(...) the symmetry or integrability conditions had frequently failed to hold, and worse, sometimes the sign differed, so the complementarity coefficient would give conflicting results when the order of calculation was inverted.

When Schultz’s tests went awry several attempt to save the Hotelling economy were made and these included, but were not limited to, changes in functional forms of the demand equations, modifications or extensions of the model to take account of possible aggregation problem or even the whole disposal of the Hotelling framework.¹⁶

What happened next is that a significant part of the dialogue lost its importance with the unexpected death of Henry Schultz in 1938, the definitive retreat of Harold Hotelling from demand theory and, indisputably the most important factor, the rediscovery of Eugene Slutsky’s, “Sulla Teoria del Bilancio del Consumatore,” published as far back as 1915, but now considered the text that provided one vital step in the proof to recover consumer’s preferences from her/his demand behaviour.¹⁷ When the Slutsky Equations finally made its way into orthodox microeconomics (in the postwar period), becoming the “single most important result” in demand theory (Hands 2004), or its “fundamental equation” (Jehle and Reny 2011), another struggle began: this time to test the Slutsky Symmetry Conditions.

Then, roughly forty-years later, and with a similar motivation, the leading empirical microeconomist Angus Deaton (1986: 1796) wrote:

All the techniques of demand analysis so far discussed share a common approach of attempting to fit demand functions to the observed data and then enquiring as to the compatibility of these fitted functions with utility theory.

For the above-mentioned compatibility to hold, integrability condition should be satisfied, which is a different way of saying that the symmetry conditions should be satisfied. In another passage, after pointing out the considerable body of empirical literature that has been carrying out the tests, including his (and John Muellbauer’s) *almost ideal demand system* of 1980, Deaton (ibid: 1791) granted:

¹⁶Drawing on the correspondence exchanged between Hotelling and Schultz, Hands and Mirowski (1998: 356-7) list ten ways the protagonists used in their attempt to overcome the failed empirical results, with Schultz always leaning toward the empirical whereas Hotelling toward the theoretical.

¹⁷See Chipman and Lenfant (2002) for how Slutsky’s 1915 and 1927 contributions grew in importance in economics. For both historical and intellectual contexts involved in Slutsky’s writings, see Barnett (2004, 2007).

Although there is some variation in results through different data sets, different approximating functions, different estimation and testing strategies, and different commodity disaggregations, there is a good deal of accumulated evidence rejecting the restrictions.

Even taking into account some of the latest development in the estimation of consumer behaviour, e.g., the nonparametric approach developed since the 1980s (Stoker 1989; Härdle et al. 1991; Haag et al. 2009) or based upon the use of nonseparable models in the 2000s (Hoderlein 2010; Imbens and Newey 2003; Altonji and Matzkin 2005), contemporary economists still fall short of attaining what Harold Hotelling and Henry Schultz wanted to attain more than eighty years ago.

4 Koopmans and the Cowles Approach to Aggregation Problems

A central claim in Wade Hands and Phillip Mirowski's recent project of re-evaluating American demand theory contends that had economists trailed more closely Harold Hotelling's demand functions, as explicated in his 1932 "Edgeworth Taxation Paradox" paper, the *neoclassical dream* would have been fulfilled. Such a dream consisted in successfully addressing six theoretical concerns within the Arrow-Debreu-Mackenzie general equilibrium model: *(i)* existence, *(ii)* uniqueness, *(iii)* stability of the equilibrium price vector, *(iv)* comparative statics via the employment of the "correspondence principle," *(v)* welfare economics, and finally, *(vi)* the satisfaction of the physics heuristics.¹⁸

But partly because of its disappointing symmetry tests and partly due to the recently discovered contributions from Eugene Slutsky, the profession walked away from Hotelling's unlimited budget approach: while Slutsky's approach turned into an essential building block of neoclassical demand analysis, the latter was relegated to the so-called "Hotelling Lemma."¹⁹

What followed in the literature is that although there appeared, "(...) three major reactions to the breakdown of the Hotelling and Schultz programme, each characterized by their treatment of the 'integrability' or [Slutsky] symmetry conditions (...)" (Hands and Mirowski 1998: 373), the representative institutions also differed in how the Slutsky-based demand functions could be used to cast light on two relevant, intertwined topics of the day: first, the Keynesian revolution and, second, the compatibility of macroeconomics (brought out by the Keynesian Revolution) and Walras's microeconomics. These two concerns continued to engage Cowles and, especially, Tjalling Koopmans over the following decades.

On the first topic, there was a general perception in the United States of the revolutionary ideas presented in John Maynard Keynes's 1936 *General Theory*. Since the interwar period had witnessed a severe economic downturn, it was feared that it could happen in the aftermath of World War II as well. Keynes's theory of aggregate income became not only an important reference to better comprehend business cycles, but was seen as providing remedies to counterbalance them. It is with this discernment that Carl Christ recalled in a 1994 article: "The applied econometric work of the Cowles Commission, inspired by Marschak and directed at the improvement of macroeconomic policy, had a definite Keynesian flavor."

Yet, Keynes's theory of aggregate income, coupled with Slutsky's contributions, enabled another much-wanted interpretation in economics: the relationship of macroeconomics to microeconomics. By breaking it out as a separate magnitude, the Slutsky income effect, considered at the agent level, provided a theoretical basis to interpret income changes at the aggregate level, as though it provided

¹⁸The general equilibrium theory represents the pinnacle of neoclassical economics.

¹⁹On this counterfactual exercise, Leonid Hurwicz (1998: 399) asserted: "Hotelling's unlimited budget model was not even under consideration for consumer choice theory, and hence there was no opportunity for rejection... Since there was no rejection, no one is guilty of it."

some sort of microfoundations.²⁰

Veering towards Slutsky and Keynes seemed a promising research strategy at Cowles: it made possible the pursuit of Alfred Cowles's and Jacob Marschak's dreams of predicting stock market and taming the business cycles, respectively (Christ 1994). In fact, as is plainly documented in the historiography, under the research directorship of Marshack (1942-1948) the Cowles Commission spent time and resources in structural estimation econometrics, precisely a macroeconomic project whose outcome went awry.

What is, perhaps, slightly lesser known is that the Commission - possibly in pursuing Cowles's desire - also engaged in applied works of microeconomic nature, a Walrasian-Slutsky program. This objective, like Schultz's efforts almost a decade before, failed as well. It did so because the estimations repeatedly rejected neoclassical price theory in the very crucial respect it rejected the Hotelling's economy: the symmetry conditions.²¹

In hindsight, given that most empirical studies have dealt (and continue to deal) with aggregate demand, could this failure be associated with a previous warning by Hotelling and Schultz, for whom the Slutsky version did not have nice properties under aggregation and, therefore, were destined to failures?

The rejections of the symmetry hypothesis in both theories made aggregation problems an even larger concern in the decade of the 1940s.²² On the substantial literature on the estimation of system of demand functions, Chipman (2006) reminded of the pioneering study of Lawrence Klein and Herman Wold (1948) and, later, of Richard Stone (1954). Around this period there also appeared programs of inquiry dealing specifically with problems of aggregation on both theoretical and empirical grounds.

In the theory realm, there was John Hicks's 1939 "Value and Capital," an attempt at constructing Walrasian macro model, "(...) a general-equilibrium microfoundational program, which is conceived as showing how Keynesian problems can arise directly from the interactions of individual agents" (Hoover 2012: 37).²³ In the empirical case, there was Lawrence Klein's 1946 *Econometrica* article, "Macroeconomics and the theory of rational behaviour," an attempt to build aggregate variables couched in the behaviours of both households and firms; this effort inaugurated, in methodological terms, a new tradition in macroeconometrics.²⁴

After Klein's article, other works followed suit, for example, in determining aggregation in one-industry model (May 1946), in building macroeconomic values in the so-called "general case" (Pu 1946), or in studying the impacts on macro values (aggregate production function) of changes in micro values (sectoral production functions), "with which it is derived" (May 1947: 51). Following May (1947), Nataf (1948) tried to determine the conditions under which an aggregate production function correspond exactly to the production functions of individual firms (rather than of isolated sectors). Analyzing instability features in systems of differential equations, Hawkins (1948) proposed

²⁰As explained in Hoover (2012), the relationship of macroeconomics to microeconomics was already an important topic in the "prehistory" of microfoundational programs, a period which included research by such economists as Ragnar Frisch and John Maynard Keynes himself. Although "microfoundations" as a systematic program of inquiry did not exist prior to John Hick's first general equilibrium approach by the end of the 1930s, and continued with Lawrence Klein's macroeconomic project of the mid-1940s and Robert Lucas's New Classical macroeconomics in the 1970s, the term was first used by a mainstream author only in the early 1970s (Phelps 1970).

²¹As claimed by Hands and Mirowski (1998: 375): "(...) actual quantitative restrictions implied by the neoclassical price rarely held in the empirical exercises undertaken at Cowles."

²²As I have mentioned in the "Introduction" to this dissertation, trade theorists have long striven with the concept of aggregation at least since Staley Jevons's concept of *trading bodies*.

²³Weintraub's 1979, "Microfoundations - The compatibility of microeconomics and macroeconomics," contains a thorough discussion of Hicks's microfoundational program.

²⁴Post-World War II macroeconometrics owes to Hicks, Modigliani and Keynes as much as it owes to Tinbergen, even though the latter two authors disagreed fiercely. Klein's innovations in the subfield came in the form of establishing a middle-ground between Keynes and Tinbergen (Hoover 2012). Fuchs-Pizón (2017) detailed the history of Klein's macroeconometrics program.

aggregation methods that could curb them. Finally, also included in these articles was a report of papers presented at a session of the Chicago Meeting of the Econometrics Society on the “Relationships Derived from Aggregate Data,” with contributions from Kenneth Arrow (1948) and Francis Dresch (1948).²⁵

To van Daal and Walker (1990), this spate of articles published in *Econometrica* between 1946 and 1949 consisted perhaps in the greatest testimony yet to the importance of aggregation problems in economics. More dramatically, still according to the authors, with the possible exception of a few remarks made in Cournot’s famous 1838 book (*Recherches sur les Principes Mathématique de la Théorie du Richesse*), no economists had ever taken the matter seriously before. One can find a similar impression of Cournot’s work, for example, in James Friedman’s (2000) survey of the French economist’s works and life.

Tjalling Koopmans’s research, on the one hand, was stirred by such an aggregation concern: a problem he decided to address by forgoing the empirical path just to adhere to the theoretical alternative. This move coincided with the dismissal of Jacob Marschak as Cowles’ research director in 1948. When Koopmans acceded to that position, rather than sanctioning research in applied works, he shifted the commission’s focus to “(...) extract abstract structure of Walrasian system and subject it to the most rigorous axiomatic scrutiny” (Hands and Mirowski 1998: 375).²⁶ An important chapter in the history of axiomatization in economics began following this event.

An illustration of how aggregation and general equilibrium theory were interlinked subjects can be drawn from Leonid Hurwicz’s appraisal of Hands and Mirowski’s alleged objectives of the Arrow-Debreu research program:

[Hands and Mirowski] impute to the neoclassicists (mainly Arrow, Debreu and Samuelson) a programme containing a set of five objectives. These are listed as existence, uniqueness and stability of competitive equilibria; comparative statics, and welfare economics (...). (*The problem of aggregation, discussed elsewhere, seems to have status akin to that of the five goals*) (Hurwicz 1998: 399, emphasis added).

But as much as Koopmans and other economists placed enormous efforts in this aggregation project, and even though “one can discern the skeleton of the Slutsky conditions buried within the [general equilibrium] system,” Hands and Mirowski (1998: 375) sustained that:

Unfortunately, axiomatic elaboration also eventually led to the conclusion that the system placed no effective restrictions upon excess demand functions, thus subverting the original impetus for the research program.

This dim view was also shared by Hurwicz (1998: 401), for whom the framework was inevitably “(...) inadequate for aggregate prediction purposes.”

Even though placed in an econometric setting, the same difficulty can be observed in a letter Koopmans wrote to Marc Nerlove after the Cowles Commission moved to New Haven (Connecticut):

(...) I have developed a general feeling that the clue to a number of different problems in econometrics lies in further exploration of aggregation problems, that ties our notions of individual decision making with somewhat more aggregated relationship. I doubt that this reflection is of much value to you in regard to distributed lags, but it does seem to me that this is one of the problems that may stand to gain if progress should be made on aggregation. Theil’s book is excellent as far as it goes. My main reservation about it is that it studies primarily the implications of current econometric procedures, rather

²⁵This annual meeting was held on December 27-30, 1947. As is customary, since the texts presented in one year are published in the following one as a joint report, the authors’ contributions were dated 1948. A third paper enunciated in the session, by Ronald Shephard, was not presented and thus did not have its abstract included in the volume.

²⁶This effort occupied most of his time until the mid-1970s.

than trying to answer the deeper question what procedures we should be using in view of aggregation problems.^{27,28}

In this vein, and also highlighting the frequency with which aggregation concerns arise in theoretical as well as applied econometric works:

The issue of *aggregation* has, I believe, special status because of its relationship to econometric research. Since much econometric research (and this, I believe, includes the studies of Henry Schultz) uses data aggregated over certain populations or collections of firms, there arises the question to which properties known to hold for individual agents carry over to aggregates. One reason why this issue is important is that identifiability assumptions used (and needed) by econometricians are often derived from theoretical (or introspective) considerations applicable to *individual* economic agents (Hurwicz 1998: 401; emphasis added).

Koopmans's research, on the other hand, was also stirred by a quarrel with affiliates of the *National Bureau of Economic Research* (NBER), one of the world's oldest business cycle research institute.²⁹ As will be shown below, this discussion encompassed the problem of aggregation and general equilibrium theory as well.

In reviewing the 1946 book, "Measuring Business Cycles," authored by Arthur Burns and Wesley Mitchell, Koopmans accused their methods of investigation as "(...) made with a minimum of assistance from theoretical conceptions and hypotheses regarding the nature of the economic processes by which the variables studied are generated" (Koopmans 1949: 161). Their theory, Koopmans reasoned, lacked an explicit formulation about the way an economic agent made choices, essential to explain "man's economic behaviour," and therefore, the ultimate causes behind the cycles.³⁰

In response to those charges, Rutledge Vining alluded to a certain vagueness in the alternative interpretation proposed by the Cowles economist, skeptical with what Koopmans thought stood behind people's choices:

Koopmans doesn't give his hypotheses specific economic content ... and suggests that the kind of content it should have in *general terms*, such as "the behaviour of group of economic agents," "underlying human responses," "knowledge of man's behaviour and its motives" (Vining 1949: 79-80; emphasis added).

That Koopmans was ambiguous when he referred to individual vis-à-vis group choices, and that he conveyed his ideas through elaborate mathematical arguments (specially after the 1960s), have already been stressed in my discussion of his writings elsewhere. Perhaps it may come as a surprise

²⁷Letter from Tjalling Koopmans to Marc Nerlove, January 31, 1957, TKP, box 16, file folder 304.

²⁸The book mentioned by Koopmans in the correspondence was Theil's "Linear Aggregation of Economic Relations," published in 1954. Henri Theil was born in 1924 in Amsterdam, the Netherlands, and passed away in the United States in 2000. After earning his Doctorate in Economics (with distinction) from the University of Amsterdam in 1951, Theil began his career as a macroeconomic analyst and forecaster for the Central Planning Bureau (CPB) of the government of the Netherlands under the directorship of Jan Tiberger. Between 1953 and 1966 he taught econometrics at Erasmus University Rotterdam (formerly Netherlands School of Economics in Rotterdam) where he also founded and directed The Econometric Institute. From 1966 to 1981 he was a member at Chicago's department of economics. From 1981 until his passing, Theil remained affiliated with The University of Florida. Over a period of five decades he published more than 250 articles and 17 books, of which the one mentioned by Koopmans was his very first (Bewley, 2000). Of all the diverse and disjoint areas theil contributed within economics, demand analysis benefited with 7 books and over 90 articles (Raj, 1991).

²⁹The NBER was established in 1919; for a historical account of its birth and later developments, see Fogel et al. 2013 and Fabricant 1984. For an early history of this and other institutions and economists that dealt with business cycles, see Morgan (1990, especially, chapter 2).

³⁰This initial denunciation by Koopmans sparked animosity between members of the Cowles Commission and of the NBER. It became known in the literature as the "measurement without theory" debate, named after the title of his article.

that such characteristics were already pointed out by Vining as early as 1949, as the continuation of his text illustrates:

But apparently *all he has to insist upon at present is the mathematical form*, and from his discussions it appears not unfair to regard the formal economic theory underlying his approach as being in the main available from works no later than those of Walras (*ibid*, emphasis added).

Another interesting feature that stands out in this passage relates to how closely connected, in Vining's discernment, Koopmans's approach appeared to be from Walras's. More comprehensively, in different excerpts, Vining spoke of Koopmans's attempt at providing aggregate formulations within the Walrasian framework. In one instance, Vining (*ibid*: 81) stated:

(...) some of us may feel that the unit of analysis and the entity the behaviour of which it is of interest to study is not the individual economizer in his conscious, problem solving state of mind. I believe that much of the statistical regularities that are to be observed in population phenomena and that are relevant for the discussion of economic problems involves the behaviour of social organisms that are distinctly more than simple algebraic aggregates of consciously economizing individuals. I think that in a positive sense the aggregate has an existence over and above the existence of Koopmans' individual units and behaviour characteristics that may not be deducible from the behaviour of these component parts.

In another passage, in criticizing Koopmans's econometric practice (inherited from Jan Tinbergen), he affirmed:

In a sense, these are the only problems that have been attacked by this entire line of development - the problem of statistical estimation that would be presented by the empirical counterpart of the Walrasian conception. Add to Walras the simple notion of lagged effects (if it is not already there) and certain devices of the nature of the difference equation, and the problem is wholly statistical as contrasted with economic (*ibid*: 80).

In closing this section, if the "measurement without theory" criticisms Tjalling Koopmans raged against the Bureau were to have some effects, he had to contribute with the Walrasian theory: building better theories of choice, perhaps with better aggregation properties, was Koopmans's task.

5 Samuelson on Homothetic Preferences, Strong Axiom of Revealed Preference, and the Representative Agent

Among the various contributions to economics, Paul Samuelson also made crucial inroads in the Walrasian general equilibrium theory. In a series of papers spanning almost thirty years, he introduced into the apparatus the assumption in which all individuals in the economy had *identical* homothetic preferences and, by doing so, he helped establish certain desirable properties in the framework.

Beginning with the article, "Social Indifference Curves," published in 1956, Samuelson adopted *homogeneous of degree one* utility functions as representation of homothetic preferences. An important aspect of this formulation is that demands functions resulting from such utility functions could be aggregated to form a *representative consumer* (or *representative agent*).³¹ As stated in Hands (2014), in addition to the existence of a representative consumer, bearing on the assumption of homothetic preferences warranted three other worth noting properties: (ii) aggregation, (iii) market

³¹See, e.g., Acemoglu (2009) and Blackborby et al. (2008) on the mathematics of the aggregation.

rationality, and (iv) welfare.³²

Because Samuelson's intention in the 1950s had been to contribute to the international trade literature, i.e., the search for community indifference curves, making a case for homothetic preferences permitted also a simultaneous re-interpretation of aggregation problems. As Samuelson (1956: 21) summarized:

Since most "individual" demand is really "family" demand, the argument can be made that such family demands have been shown to have none of the nice properties of modern consumption theory. However, if within the family there can be assumed to take place an optimal reallocation of income so as to keep each member's dollar expenditure of equal ethical worth, then there can be derived for the whole family a set of well-behaved indifference contours relating the totals of what it consumes: the family can be said to *act as if* it maximizes such a group preference function. The same argument will apply to all of society if optimal reallocation of income can be assumed to keep the ethical worth of each person's marginal dollar equal.

It is, therefore, precisely due to this extension that the Walrasian general equilibrium model:

(...) did have sufficient structure at the *agent level* to be able to say very specific (and desirable) things about the *market-level* results generated by the competitive interaction of such agents. The model was the homothetic Santa Claus case of uniform homothetic tastes...(Hands 2014: 427, emphasis added).

Along with this aggregative property, the existence of the representative agent meant that rather than a *supply = demand type equilibrium* (for all goods in the economy), now it sufficed to arrive at the solution to a constrained optimization problem of that single agent. As a consequence, following all usual assumptions of demand theory, the rationality of a single individual is (as if) transferred to the whole system.³³

Parallel to this event, in separate efforts, Samuelson (1950) and Houthakker (1950) developed the *Strong Axiom of Revealed Preference* (SARP) approach to consumer theory, one which emboldened the perception that a representative agent could in fact exist in the economy: "(...) if the SARP axiom holds on market (rather than individual) demand functions, then there always exists a rationalising representative agent: i.e., the so-called Wald case where the market reflects 'revealed group preference' (...)" (ibid: 419).

Contrary to the reliance on the *weak axiom of revealed preference* (WARP), the fulfillment of SARP signified the satisfaction of integrability condition necessary for the demand function to be treated as if it were generated by a budget-constrained utility maximizing agent. Borrowing a last passage from Wade Hands (2014: 430), he affirmed:

(...) if the SARP holds on market demand functions then the demand functions behave as if they were generated by a representative agent and the Walrasian general equilibrium of the whole economy reduces to the consumer's equilibrium of that agent.

An important point here is that more than enabling the construction of a particular breed of general equilibrium model, the elimination of heterogeneity of agents and income effects - as a result of homothetic preferences - "liberated" the representative agent to other fields of economics as well,

³²All four implications following the assumption of homothetic preferences are unique in their importance to the general equilibrium model. However, to the question I am raising in this text, the most important one is the possibility of considering the market demand as if generated by a single, representative consumer.

³³The attainment of the representative agent means also the solution to two other specific problems within general equilibrium theory: uniqueness and stability. However, I will not pursue such topics here.

such as growth theory, international trade, and financial economics.³⁴ As a matter of fact, in another article I investigate how, if at all, Samuelson's representative agent, as used in macroeconomics, made it to Koopmans's: it turns out that the channel of influence might have played out in the subfield of optimal growth theory. Before, I shall discuss a change in the meaning of dynamics as portrayed in Hands (2010).

6 The Postwar Stabilization of Consumer Choice Theory: redefining dynamics

It seems that with regard to the stabilization of consumer choice theory, the thesis of a "skein," or of an "interlocking competitive system," consisting of theories that culminated in the three American neoclassical hubs (the Cowles general equilibrium approach, the Chicago Marshallian variant and the Samuelson's revealed preference doctrine) has been virtually uncontentious.³⁵

In an article published in the *European Journal of the History of Economic Thought* in 2010, Wade Hands introduced into the former interpretative thread yet another simultaneous modifying feature: it ceased to be dynamic. This time, the source of amendment came from another series of contributions by Paul Samuelson. Notably, was his PhD dissertation, "Foundations of Economic Analysis," published in 1947, a source that played a key role.³⁶ In his appreciation of Samuelson's contributions, Hands (2010: 332) wrote:

By the 1950s "dynamic" meant "based explicitly on differential or difference equations involving time," and optimization problems - maximum or minimum - were *not* of this sort. Maximization was not a dynamic process; the Walrasian tâtonnement was.³⁷

In another passage, Hands claimed that:

The ultimate impact of the separation - or the impact of the profession generally accepting this separation - was that consumer choice theory, which was based on utility *maximization*, ceased to have anything to do with movements or dynamics. Of course no dynamics means no paths, no endowment effects, no reference dependence, no order of consumption, none of the other problems associated with integrability_B. The concept of economic dynamics is stabilized and in the process consumer choice theory is relieved of the responsibility for dealing with all of these potentially troublesome issues. Economic agents with well-ordered preferences defined over the entire choice space became the standard basis for consumer choice theory, and the non-integrable case and all the difficulties associated with it quietly left the stage. Stabilizing dynamics thus helped stabilize consumer choice (Hands 2010: 332).³⁸

If one were to follow Hands's classification on what (additionally) might or might not belong to the stabilized body of consumer choice theory, Koopmans's articles in intertemporal economics would not

³⁴"Financial Economics" studies the interactions of households, firms and financial intermediaries in both domestic and international settings. In the latter case, national economic policies might be even more important a force. Robert C. Merton defines it as the overlapping of finance, monetary economics and public finance.

³⁵As far as the Hotelling-Schultz impasse is concerned, there has not yet appeared a challenging thesis disputing its validity, with the exception of Hammond (2006), who disagrees with chief aspects concerning the unfolding of events from the standpoint of the Chicago School.

³⁶In Wade Hands's argument, since Samuelson's 1941, 1942 and 1944 articles on stability were all incorporated into his 1947 volume, it suffices to refer only to this latter work as his main contributions to the topic. Furthermore, on the simultaneity issue, such contributions by Samuelson coincide temporarily with the formation of the "skein," as defended by Hands and Mirowski (1998) and Mirowski and Hands (1998).

³⁷So clear was the message that Samuelson wanted to convey in his book that he separated it into Part I, dedicated to optimization problems, and Part II, dealing with dynamics.

³⁸For an explanation of Integrability_B, see Hands (2006).

pass the test. That is to say, Koopmans continued to carry out his studies in the 1950s and 1960s the “old” way, searching for a dynamic nature within it even after a possible separation between optimization and dynamics became more visible in the literature.

Even though it is possible to insist that Koopmans did not undertake dynamic analysis proper since neither differential nor difference equations were used, Hands’s exemplification quoted above, wherein “well-ordered preferences defined over the entire space became the standard basis,” cannot be used to shed light on Koopmans’s contributions either. As I have treated with some details elsewhere, a crucial part of his analysis was shore up on the notion of impatience and time perspective, thus, making time a crucial matter.³⁹ Furthermore, as it has also been shown, Koopmans continued to pursue a distinctive preference-based demand theory in the 1960s by postulating preferences orderings *over time* rather than *on a prospect space*. For several authors, such a distinction cannot be easily applied either.

In his instructive, “Applied Consumption Analysis,” all chapters concerning Koopmans’s contributions to consumer choice theory were allotted to a whole section titled “dynamics.” In appraising the case in which the consumer looks into the future, Louis Philips stated:

We want to explain the allocation of his budget among n commodities, when due attention is given to the fact that he is not maximizing an instantaneous “static” or an instantaneous “dynamic” utility function (in which the influence of the past behaviour is incorporated) as in previous chapters, but is maximizing an “intertemporal” utility function defined on sequences over time (from now to some future date) of commodity bundles (Phlips 1983: 267).

Judy Klein (2007) made the case in which Richard Bellman used dynamic programming intensively to carry out “economizing” efforts during the second World War. Economists then began to apply the same functional equation formulation first in microeconomics and, later, in macroeconomics. A successful incursion of economic dynamics into the realm of microeconomics would surely have required its application in consumer choice theory. Lars Peter Hansen (2010) argued that the first use of recursive preferences can be traced to Koopmans (1960) and Koopmans et al. (1964). Stokey et al. (1989) added to this list the article by Beal and Koopmans (1969).

Following this reasoning, considering that for Shone (2002: 3), “(...) dynamics involves time derivative, dx/dt , where x is a continuous function of time, or difference equations, $x_t - x_{t-1}$ where time is considered discrete units,” and, in the context of a neoclassical growth model, Corbae et al. (2009: 92) asserted that, “A dynamic optimization problem is one in which a decision must be made over time in which early decisions affect later options,” it seems hardly straightforward the rupture as proposed by Wade Hands.

Although providing a historical account that cannot be taken at face value, on the inclusion of dynamic (and stochastic) features into economics, in the graduate text, “Recursive Methods in Economic Dynamics” one can find:

These theoretical developments are based on a wide variety of results in economics, mathematics, and statistics: the contingent-claim view of economic equilibria introduced by Arrow (1953) and Debreu (1959), the economic applications of the calculus of variation pioneered long ago by Ramsey (1928) and Hotelling (1931), the theory of dynamic programming of Bellman (1957) and Blackwell (1965) (Stokey et al. 1989).

The passage shows that at least some of the publications that contributed seminally to dynamics, and which were later applied to consumption theory, e.g., Bellman (1957) and Blackwell (1965), came out at the approximate time of their alleged separation (stabilization).⁴⁰ Finally, it can be argued

³⁹See the article, “Preference, Utility, and Koopmans’s Stance on the Representative Consumer.”

⁴⁰In the case of Ramsey (1928), there is even a clearer intersection between dynamics and choice theory.

that dynamic optimization techniques solve problems posed, for example, in Hamiltonian dynamic system and they date back at least to Samuelson and Solow (1956) (Wulwick, 1995).

In different ways, the excerpts above coincide with a key economic substance in Koopmans's research, that is, the blurring of optimization (choice theory) and dynamics (intertemporal features). One can discern this feature in the articles written in the 1950-1970 period as well as in the only application he made of that theoretical framework: the 1963 normative growth model.

7 Concluding Remarks

Casting new light on the history of demand theory serves as much the writing of more accurate account of this episode as the building of a firmer ground upon which a better comprehension of two interwoven facts can be built: the stability of general equilibrium theory and the greater appearance of the representative consumer in the 1950s and 1960s. Considering that this period also coincided with the rise of optimal growth models, where infinitely-lived agents play an important role, I have tried to offer an overview into the history of demand theory whereby all these topics could be more clearly delineated.

Even though it was not my aim to emphasize the complex Hotelling and Schultz dialogue, it is worth quoting a passage from Mirowski and Hands (1998) for two reasons. In addition to conveying precisely the methodological and economic significance of Samuelson's contributions to the impasse, the passage *per se* also helps us observe the state of demand theory at that time, and hence, useful for comparing what the Cowles Commission used to be at the end of the 1930s and what it has become after an important person in this essay, Tjalling Koopmans, took over the research directorship of the institution in 1948.

The problem for Samuelson was to find a way of formulating the theory of demand that would be consistent with his positivist-operationalist methodology while simultaneously *avoiding the type of econometric testing* associated with Schultz. How could this possibly be done? (...) In essence his answer was to *change the place where the empiricism lived in the neoclassical theory of demand*. Instead of having empiricism enter at the back end - by testing the empirical implications deduced from theory - the revealed preference approach would place empiricism right up front at the beginning of the exercise. If the epistemologically dubious notion of subjective utility could be replaced with a strictly behaviorist - thus objective, observational, operational, and meaningful - concept of consumer action, demand theory could be reconstituted on what Samuelson considered legitimate scientific foundations (Mirowski and Hands 1998: 283, italics added).

In other words, in the 1930s, and for most part of the 1940s, demand analysis was overwhelmingly an econometric enterprise.

An important remark in this article, therefore, is that if the Cowles Commission carried out applied econometric projects of microeconomic and macroeconomic natures before Koopmans, after his appointment as research director in 1948 the institution was transformed into a fulcrum of pure theoretical microeconomics. More important, motivated by aggregation discussions taking place in economics, the new research program at Cowles was also directed to tackle the problem.

I have also stressed in this paper that an significant portion of this discussion, of compatibility of micro to macroeconomics, can be traced to another historical fact: the widespread adoption of homothetic preferences, first introduced by Paul Samuelson in 1956. Then, I have I showed that Koopmans's research purpose intersected with Samuelson's push for homothetic preferences.

Although consumer theory and dynamics stabilized as two separate branches within microeconomics in the 1950s and 1960s, Koopmans still tried to extend the first subfield through the addition of dynamic features, initially with respect to the structure of utility functions and, later, with respect to preferences. This observation, however, seems to be at odds with interpretations given, for

example, in Hands (2010) and I provided some examples in the literature showing why it is hard to tell consumption theory and dynamics apart.

I hope the narrative in this article has identified few junctures that made more transparent the relationship between the development of demand theory and the rise of the representative agent in microeconomics.

At least two important questions remain open for further investigations. The first one concerns Koopmans's own use of homothetic preferences and how much he interacted on this matter not only with Paul Samuelson, but with Gerard Debreu, an important mathematical economist who made essential contributions to the axiomatization of economics and who was also a member of the Cowles Commission. A second question concerns a more clear association between the Koopmans-Samuelson writings in homothetic preferences and the time-separability preferences used in modern macroeconomics as discussed in Barro and King (1984).

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